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ON ORGANIC PHYSICS.

By Henry Hartshorne, M. D.

(*Read before the American Philosophical Society, Jan.* 19, 1872.)

In the title chosen for this paper, there may appear to be something anomalous or contradictory: organic science and physical science being commonly regarded as almost incommunicable departments. But as we have long had, already, organic *chemistry* and animal *mechanics*, the tendencies of opinion, and to some extent the clearly rational interpretation of facts, now favor the re-inclusion of organic natural science under the wide term physics, from which it became long ago separated upon grounds of theory, and as it may still continue to be, for the classifications of convenience.

The proneness for unification, so natural to human intelligence, being methodically sanctioned under what is called the "law of parsimony," the question from period to period in all fields of thought is, how far can we *legitimately get* in our simplifications and unifications? Provisionally, at least, we must mark our steps; as, what is done in these unifications of science is to aim at the "reduction of our complex symbols of thought to the simplest possible" *appropriate* "symbols." When the alchemists thought that they might transmute or reduce all elements to one, they did not succeed. There are now some theorists who would reduce all our ideas of law, order, and causation in nature to the one idea of *continuity.* I believe that they will not succeed, in the end, better than the alchemists. This endeavor is, just now, being made especially in the region of life; and while, as already said, there is enough to sustain fully the proper *inclusion* of vital phenomena along with the other phenomena of nature as *physical*, I hold that there is not enough to establish this *identity* or immediate continuity, in the sense in which it has been asserted by some biologists. Let us address ourselves for a few moments to the elements of this question in its recent aspects.

First: What do we mean by life? What is it that we are to regard as differentiated from, or identified with, the other physical forces of nature? We may drop out at once the old idea, that all actions of a living body, such as digestion, circulation, aeration, and the rest of the *special* functions, are properly called vital; or that these need to have any peculiar or *specific* force, or phase of force, to explain them. Digestion is chemical; circulation, mechanical, and so on. By proper exclusion, then, we come down to this: that only one (perhaps a two-fold) process is truly vital, in the sense that its facts come under the category of no other force of nature, under no other name hitherto known to science, but that of life. And this process is *assimilation*, with type-formation or definite organization as its result: the segregation of matter in a peculiar molecular state, whence comes its assumption of peculiar though rapidly changing forms; the chemical instability of the matter being in direct correspondence with the mutability of its forms. Life is then a change, a molecular motion; and it needs a name for itself as distinctly as heat motion, electrical, or any other kind of movement. But, is it the direct resultant of those other movements? Is organic evolution simply in the line or plane of the composition of the cosmic forces, gravitation, heat, light, electricity, magnetism, chemism, so as to be the mere outcome of these? I would say, no. And this is a very different thing from denying the *correlation* of the vital and physical forces. They are clearly correlative; but correlation is not indentity. And the question is a deep one, what their exact relation is; it is now one of the cardinal questions in science. Because of the wide variety of its bearings, let us regard their most general aspect first.

Grant, as a postulate, the "conservation of force." Then there follows: 1st, as its corollary, that not only is the total of force in the universe never diminished, but, conversely, this totality is never, by physical causation, *increased.* As apparent exhaustion of force is only its transmutation, so apparent increase of it must also be only transmutation of it.

2d. No *change,* in the nature or direction of any force, can, in accordance with the second law of motion, be either uncaused or self-caused, that is caused by the force itself. Every such change must have a special, sufficient cause. When, then, the laws and tendencies of gravitative, electric, magnetic, chemical and heat and light forces are known, and are found to promote, by preference in all instances, the formation of compounds of *stable* equilibrium, by the union of carbon, hydrogen, oxygen, sulphur, and phosphorus, etc., we must expect this to be *uniformly* the result of their direct action. And, therefore, when we find conspicuously unstable compounds to be formed of those same elements, although in the presence of the same general forces, we ought to conclude the formation of such compounds to be the result of *another* definitely acting force. When the force x has been proved always to act in the line A B, and the force y to act in the line C D, we must be sure that a force acting in the reverse line B A, or D C, or in any line intermediate between B A and D C, cannot be either the force x or the force y, or a force resulting from the composition of X and Y, but must be another force, say z. Assuming the existence of z, we may then endeavor to ascertain its relations to the other forces; and we may also inquire whether there may or may not be still other forces of whose composition it is the resultant. So we ought to conclude that there is a special force, or mode, or line of force, whose action is the assimilation and new construction of organizable, protoplasmic, or bioplasmic matter, because the action so named involves a movement of elements in direct opposition to that produced by the other known forces, as shown especially and invariably by the action of those forces upon the same matter, when death occurs.

I may introduce here a remark upon the chemical part of the discussion; whether, as a matter of *induction,* all *a priori* reasoning apart, we are warranted in saying that organizable matter is and can be never produced by chemical or any other general cosmic forces. Since Wöhler's first formation of urea from carbonate of ammonia, many years ago, this has been much argued upon. Numbers of organic substances have since been made by Berthelot and others, by more or less nearly direct synthesis. But mark this. With the single exception, *possibly*, though not probably, of neurin (that exception remaining very doubtful yet, especially as the substance so designated as made by synthesis, is reported to be crystalline or crystallizable), all the compounds so formed are not organizable, but what I would call *post-organic* substances, products or educts of retrograde or downward metamorphosis; excreted or secreted,—made in animal or vegetable bodies not by their life-force as such in tissue formation, but by its "composition" or balance *with other forces*, in the retrogressive metamorphosis, the approach toward waste and death. It is not *germinal* but effete *formed* matter, to use the words of Beale. Such, for instance, is even quinia, though not yet made by synthesis. Such may be neurin itself, in the form in which we get it after the death of an animal, since no tissue is more prone to change soon after death than the nervous. In fact, if with Lehmann, Moleschott and Huxley, chemists should assert that life is only a *property* of certain substances, and, as some chemists at least say, that those substances can be made in the laboratory, then we must hold them to the test; and deny the formation of any of those substances themselves, *until they are shown to manifest all their properties, including life.*

Time cannot be allowed in this paper, more than to allude to the present aspect of the closely connected question as to the evidence of experiment, in reference to the origination of minute forms of life. After the controversy (which was very active in the day of Crosse's electrical experiments) had, by elimination, been narrowed down to a chronic debate between Pasteur and Pouchet, we are now surprised by its assuming larger proportions, with Owen, Clark, Hughes Bennett and Bastian, coming out as decided heterogenists, or advocates of abiogenesis. Similar observations and like arguments, however, come up again and again. No better case has ever been made out for heterogeny than by Charlton Bastian in his papers in "Nature," 1870. I need not dwell on the known difficulty of exactness in such experiments, from the first preparation of the apparatus down to the last examination of the results under the microscope: a difficulty, as regards the total exclusion of foreign particles, pronounced by some experts to be insurmountable.

We need only observe here that Bastian's protest against Pasteur's assumption, that the prior weight of improbability is *against* the heterogenic theory, is not warranted; as the burden of proof certainly does rest with the heterogenists. It being perfectly well known that no experience exists of the beginning of life of *larger* forms, however simple, without parentage, we may say that since nothing larger than $\frac{1}{1000}$ of an inch has ever been known to begin life in an entirely inorganic medium, the probability is vast that nothing *smaller* than $\frac{1}{1000}$ of an inch diameter ever does so. While we are not able to say that it is *impossible*, those who assert it must bring most cogent evidence. And the legitimate *alternative*, in regard to all the *heating* or boiling experiments, is, whether we are to apprehend in them new evidence of the resistance of some low living forms to the usually destructive influence of heat, or to assume the total effect of this, and then conclude that a *de novo* origination of life has really occurred. There are many facts which sustain the choice of the former interpretation; facts, for instance, concerning confervoid vegetation in boiling springs, and such as those observed by G. Pouchet and others, proving the marvellous tenacity of rotiferous animalcular life. Jeffries Wyman's experiments, moreover, seemed to show, that though four hours would not, five hours boiling would, prohibit the appearance of any vitality in the materials under his examination. Frankland and Calvert have since strongly confirmed the same conclusion by their careful experiments. No one, however wedded to heterogeny, can say that we are yet at the end of our knowledge of the limits of vital resistance to physical agencies, including heat. But note further: that putting aside (although Professor Huxley does not) as unjustifiable, the supposition that it was possible for an observer like Bastian to be mistaken as to the really organic character of the very minute filaments and sporules, observed by him in tartrate of ammonia or other solutions, under restricted conditions, we must find in them at least something extraordinarily different from the life which we are accustomed to observe; since a very important part of the whole process was the entire exclusion of air; so that if there was life, it was such only as could exist in distilled water, or *in vacuo*. And here, just as in the controversy upon the essential distinctions between animals and plants, since deoxidation and oxidation, fixation of carbon and its elimination, are directly opposite processes, though we may not yet find their separating line, still the line must *exist somewhere*. And it would be taking for granted a great deal more than any evolutionist has a right to do as yet, to suppose, not only that Bastian had thus manufactured sporules and filaments living in airless tartrate of ammonia, but that all he would need would be some greater variety of conditions and time to evolve from them the whole system of organized nature. Bastian himself has not yet asserted this.

A word more about the above named *opposition* between vital and other cosmic forces. It may be stated thus: According to the nebular theory mostly accepted now as the basis of cosmogony and evolution,—the formation of the worlds of our solar system has been and is attended constantly by the integration of matter and the dissipation of force. I have, in this expression, used Hebert Spencer's words. The spheres in consolidating from diffused nebulous matter *give out* force as heat. But, per contra, *organized beings* integrate matter, and *at the same time accumulate force*. In the language of Professor Barker's able discourse on the Correlation of Vital and Physical Forces, "the food of the plant is matter whose energy is all expended; it is fallen weight. The plant-organism, in a way yet mysterious to us, converts the actual energy of the sunlight into potential energy within it. The fallen weight is thus raised and energy is thus stored up."

As Dr. Barker adds, the force which is stored up is undeniably physical; but I remark further, that the process *by which* it is *stored* is of another order, and involves a different kind of physical force movement from that by which it is *evolved* and expended.

What more can be made out about this mysterious force of life? Not much as yet; but enough, perhaps, to give encouragement to investigation. Reduced to its simplest element, namely, the cell, or the physiological unit, life is a process of *incretion* and *excretion*. What goes in as food is made into tissue, and (after functioning) comes out as waste. The functioning is secondary to it as *life*, though no doubt in itself primary under the view of purpose. The organic cell converts crystalloid atoms or molecules into colloid molecules. Now what is the difference? not merely in the fact of different degrees of diffusibility, but in the state of the particles; the reason why they are colloidal? May we not conjecture, that it may be owing to a differentiated movement of the *atoms?* Clausius and others have long since given reason for supposing that the particles of all gases are in incessant motion among themselves. May not these atomic movements of the three gases, nitrogen, hydrogen and oxygen, all of which are associated with carbon in bioplasm, be in some manner retained in the integration of the organic cell?

Life-motion is probably not *undulatory*, like light, heat and electrical movement, but rotary or cycloidal. For an analogy, I would allude, somewhat too boldly, to the theory of some astronomers, of the present constitution of Saturn's rings; of multitudinous small masses incessantly in motion among themselves. And the occasion for brevity in this communication must be the excuse for crowding, before I conclude, yet other questionable proposals of analogy; as of the minute microscopic cell, with its inward, and outward currents through undiscoverable pores, with even the incretions and excretions of the sun and its photosphere; whose outward and inward cloud-currents are now being so laboriously studied. Somewhat less remote, certainly, is the suggestion of analogy (*not* identity) of life-actions with effects of some of the other forces of physical nature. I would regard sexual union which (except in mere *dividuation*, such as the life of a tree in its cuttings, or the fissiparous generation of animals) is, until heterogeny or spontaneous generation be proved, the only method of the indefinite continuation of life,—I would regard this sexual union as the true analogue of chemical union. The importance of bi-sexual polarity in organic nature has hardly yet been fully appreciated. Carbon and oxygen uniting give out *heat*, and carbonic acid which escapes. Organisms of two sexes, say the pollen cells and germ cells of a plant combine, and evolve *life*-force, whose products do *not* escape, but *remain* as organizable and organized protoplasmic matter; developing new cells in connection with each other. Yet another analogy with physical force-action may be presented. It is known that phyllotaxis, or the leaf and branch arrangement of plants, follows a spiral law, arithmetically calculable, and showing a striking correspondence with the order remarked in the successive distances of the planets of our solar system from the sun. But what this phyllotaxis still more readily recalls to us is,—the helix of the electro-magnet; or, rather, of the magneto-electric apparatus. As the opposite polarities of the magnet are to the current of the helix of wire, so may be—of course, we do not say is—sexual bi-polarity to the spiral phyllotaxis. While a spiral tendency or movement cannot be so clearly traced in animals, yet some indices of what we may call *organo-taxis* are not wanting. As opposite leaves are held to represent a double spiral, and whorls two or more, so the bilateral symmetry of vertebrates, articulates and some mollusks, and the whorled form of radiates and cœlenterates, may present or imitate results of a similar polar force.

Another analogy, with which physicists may have more patience, is of a *reverse* kind, with heat. As a spark of fire sets burning an indefinite amount of combustible matter within its influence, so a spark of life vitalizes successively an indefinite amount of viable, organizable material as food. But the difference is, as remarked already, that while the increment of heat-force instigates the continuous *reduction* of less stable conditions of matter to those more stable, the increment of life-force *elevates* materials from stable into unstable substances, with constantly transmuted forms.

To conclude: By no such crude analogies as these can any one imagine that the mystery of life is to be altogether removed. These remarks are presented mainly to suggest and show that inquiry into life-force and its attributes may now legitimately follow methods like in nature those used in studying the other physical forces; and to expand to some extent the germinal thought, that, while life or life-force must yet be always differentiated from the other cosmic forces, it is, like them, a motion, or mode of motion, whose study is a part of physics—organic physics.

I would add that such a view of the correlation of biology with the other physical sciences no more interferes with a theistic and teleological view of creation than does the (now familiar) resolution of many once called vital actions (as digestion, circulation, blood-aëration) into chemical or physical acts, the results of ordinary forces of nature, which are *collocated* in the animal body under the conditions of vitality. To analyze is not to create, or even to show how creation was effected; much less is it to afford a negation of the fact of creation itself. Yet, to analyze is always legitimate in science, so long as it is done accurately, step by step; and this, whether it point to biogenesis or abiogenesis, to the origin of types by interrupted appearances or by evolution.

The discussion of Mr. Price's paper, read at the last meeting, being in order, Mr. Cope made the following remarks:

As the essay read before this Society under the above title, adduces no facts for or against the theory of Evolution not already known, the writer does not propose to criticise it as a whole. His object is to correct some statements of supposed fact, which are germane to the argument of the essay, in which its author has been led astray by the statements of others, or want of familiarity with the subject.

The erroneous statements are the following :

1st. That the gradations of variational and specific form seen among domesticated animals are peculiar to them, not being found among animals in the wild state ; and are therefore due exclusively to the influence of human interference.

2d. That fertile hybrids do not exist in the wild state, and that their existence between domesticated varieties is therefore evidence of their common origin.

3d. That transitions from species to species, as to form and other essentials, occur neither in the present period nor in the succession of geologic strata.

4th. That the examples of intermediate forms, or supposed transitional structures, on which the evolutionists rely, are abnormal or monstrous, or otherwise insufficient for the use made of them.

These are four very natural popular fallacies, and the present seems to be a suitable opportunity for exposing them.

First. That graded varieties and unstable specific forms do not exist outside of domestication, and are due to its influence, etc. To find an unqualified contradiction to this statement, it is only necessary to refer to the diagnostic tables and keys of the best and most honest zoölogists and botanists. It is true that these diagnoses are dry reading to the non-professional, yet they embrace nearly all that is of value in this part of biological science, and must be mastered in some department, before the student is in possession of the means of forming an opinion. The neglect to do this explains why it is that, after all that has been written and said about *protean* species, etc., the subject should be so little understood. The fact is thus : That in every family or larger group of animals and plants, there exists one or more genera in which the species present an aggregation of specific intensity of form ; that is, that species become more and more closely related, and, finally, varieties of single species have to be admitted for the sake of obtaining a systematic diagnosis, which will apply to all the individuals. These varieties are frequently as well marked as the nearly related species, so far as *amount* of difference is concerned, the distinction between the two cases being that in the varieties there is a gradation from one to the other ; in the species, none. Nevertheless, between some of the varieties, transitions may be of rare occurrence, and in the case of the "species," an intermediate individual or two may occasionally be found. Thus it is that differences of the varietal and of the specific kinds are distinguished by degree only, and not in kind, and are therefore the results of the operation of uniform laws. Yet, according to the old theory, the varieties have a common origin, and the species an independent one. To cite examples of what is asserted, the monograph of the Tenebrionidæ, by Dr. Horn, in the Transactions of this Society, especially the genus *Eleodes*, may be mentioned, or the essay on the genus *Ptychostomus*, in the writer's "Fishes of North Carolina," in the Proceedings of the Society, may be consulted.

It is true that in but few of these cases have the varieties been seen to be bred from common parents, a circumstance entirely owing to the difficulties of observation. The reasoning derived from the relations of differences appears to be conclusive as to their common origin, unless we are prepared to adopt the opposite view that the varieties have originated separately. As these avowedly grade into individual variations, we must at once be led to believe that individuals have been created independently: a manifest absurdity.

But variations in the same brood have been found among wild animals; for example, both the red and gray varieties of the little horned owl (*Scops asio*) have been taken from the same nest.

As further examples of gradation between species and varieties, found in nature, I only have to select those genera most numerous in species and *best studied.* Among Birds: *Corvus*, *Empidonax*, *Buteo*, *Falco*, etc.; Reptiles: *Eutænia*, *Anolis*, *Lycodon*, *Naja*, *Caudisona*, *Elaps*, *Oxyrrhopus*, etc.; Batrachia: *Rana*, *Hyla*, *Chorophilus*, *Borborocoetes*, *Amblystoma*, *Spelerpes*, etc.; Fishes: *Ptychostomus*, *Photogenis*, *Plecostomus*, *Amiurus*, *Perca*, and many others.

These *protean* genera are not the majority of those known to naturalists, but are quite numerous. That the variability depends on a peculiar condition of the animals themselves, and not on domestication, excepting in so far as it produces these conditions, is plain not only from the above facts, but from those observed in domestication. It is well known that while pigeons, fowls, cattle, dogs, etc., are very variable or "protean," the pea-fowl (*Pavo*) has maintained its specific characters with great accuracy, during a period of domestication as long as that of the other species named. The same may be said of the Guinea (*Numidia*) and the Turkey (*Meleagris*). These facts show that domestication is only a remote cause of variability.

Second. That hybrids do not occur among wild animals, etc. The affirmative of this question is no more important to the view of evolution than the reverse; nevertheless, if hybridization be regarded by any as evidence of common origin, as the author of "Phases of Modern Philosophy," etc., believes, then some wild species are undoubtedly descended from the same parents. There are a few fertile hybrids in nature, though some animals have been stated to be such without sufficient evidence; for example, the *Colaptes ayresii* (woodpecker) is thought by Professor Baird to be a permanent hybrid between the Eastern *C. ornatus* and Californian *C. mexicanus*, and as it occupied the region between the two (Upper Missouri) there is every reason to believe that such is the case, especially as it mingles and breeds freely with both the others on the borders of their range.

Third. That transitions from species to species in the present periods have not been observed; nor have they been discovered in passing upwards through strata of the earth's crust.

The all-sufficient answer to this statement is to be found in the imperfection of our system of classification already pointed out. Thus, if we first assume with the anti-developmentalist, that varieties have a common parentage, and species distinct ones, when intermediate forms connecting so-called species are discovered, we must confess ourselves in error, and admit that the forms supposed to have had different origin, really had a common one. Such intermediate forms really establish the connection between species, but the question is begged at once by asserting unity of species, and therefore of origin, so soon as the intermediate form is found; for, as before observed, it is not degree but constancy of distinction, which establishes the species of the zoölogical systems. Transitions between species are constantly discovered in existing animals: when numerous in individuals, the more diverse forms are regarded as "aberrant;" when few, the extremes become "varieties," and it is only necessary to destroy the annectant forms altogether, to leave two or more species. As the whole of a variable species generally has a wide geographical range, the varieties coinciding with sub-areas, the submergence or other change in the intervening surface would destroy connecting forms, and naturally produce isolated species.

Formerly, naturalists sometimes did this in their studies. A zoölogist known to fame, once pointed out to me some troublesome specimens which set his attempts at definition of certain species at defiance. "These," said he, "are the kind that I throw out of the window." Naturalists having abandoned "throwing" puzzling forms "out of the window," the result of more honest study is a belief in evolution by four-fifths of them.

Fourth. That the "variations" or intermediate types pointed to by evolutionists in favor of their positions, are exceptional, abnormal, or too few to be available in demonstration of the origin species in general, etc.

The cases of transition, intermediate forms, or diversity in the brood, observed and cited by naturalists in proof of evolution, are few compared with the numbers of well defined, isolated species, genera, etc., though far more numerous than the author of the article criticised is aware of. Their value in evidence of the nature and origin of the permanent forms, is, it seems to me, conclusive, and to a certain extent, complete. By the inductive process of reasoning we arrive at a knowledge of the unknown from the known, a process which we act upon in our daily affairs, and one which constitutes the key to knowledge. It rests upon the invariability of nature's operations under identical circumstances, and for its application merely demands that analysis and comparison shall fix that the nature of that of which something is unknown, is identical with that of which something is known. We then with certainty refer that something which is known, as an attribute of that object of which the same quality had been previously unknown.

In application to the question of evolution the following preliminary facts may be assumed:

1. Many species are composed of identical elemental parts which present minor differences.

2. Some of these differences have been seen to arise "spontaneously;" that is, characters have made their appearance in offspring of parents which did not possess them,—or what is the same thing, are known to exist in individuals whose parentage is identical with others which do not possess them.

3. The gradation of differences of the same elemental parts is one of degree only, and not of kind.

4. Induction: Therefore all such differences have originated by a modification in generation, or have made their appearance without transmission, in descent.

This induction is one of the forms in which the proof for evolution appears, though a more cogent argument is that already presented in Chapter I, of the paper entitled "On the Method of Creation," etc., Proceedings A. P. S., 1871.

The fact that in the majority of species, their origin by descent with modification has not been directly observed, in no wise invalidates the above argument. Unless they present positive evidence against such origin, these are absolutely silent witnesses. He who cites them against evolution commits the error of the native of the Green Isle, who was present at a murder trial: "Although the prosecuting attorney brought three witnesses to swear positively that they saw the murder committed, I could produce *thirty* who swore that they did not see it done!"

Finally, it may be asserted that the Theory of Development is the only theory of creation before the scientific world at the present time. The author of "Some Phases," etc., says, in opposition to it, that *God made* the species, and that their gradual evolution dispenses with His interferences and authorship. Will our author explain to us *how* God made the species independent from the start? No opponent of development has attempted to do this, and until it is done, there can be no theory or doctrine in the field other than that of Evolution. The Evolutionists not only believe with the author criticised, that God made all things, but they attempt to show in the field of biology, *how* He did it.

If they are correct in their interpretation of the facts, there can be, and is, no interference between their views and the purest morality, and the most faithful religion.

Other members took part in the discussion.

On motion, Mr. Lesley was elected Librarian for the ensuing year.

On motion, the nomination of Standing Committees being in order, those of the preceding year were continued.

On motion, the reading of the Catalogue of Members was dispensed with.

Pending nominations Nos. 679, 689, and new nominations 690, 691 were read.

Nominations Nos. 678 to 688 were spoken to by Nominors and ballotted for; and on a scrutiny of the ballot boxes, by the presiding officer, the following gentlemen were declared duly elected members of the Society:

Prof. W. C. Kerr, State Geologist of N. Car., Raleigh, N. C.

Mr. La Motte Dupont de Nemours, Wilmington, Del.

Prof. William P. Trowbridge, of Yale College, New Haven, Conn.

Dr. William Elder, of Philadelphia.

Francis Bowyer Miller, Esq., of the Royal Branch Mint at Melbourne, Aus.

Mr. Guillaume Lambert, Professor of Chemistry in the University of Louvain, Belgium.

Mr. Persifor Frazer, Jr., Assistant Prof. Chemistry, University of Pennsylvania, Philadelphia.

Mr. George W. Hough, Director of the Dudley Observatory, Albany.

Mr. William A. Stokes, of Philadelphia.

Mr. Edwin J. Houston, Professor in the High School and Franklin Institute, Philadelphia.

And the Society was adjourned.

Stated Meeting, February 2d, 1872.

Present, fifteen members.

Vice-President, Mr. **Fraley**, in the Chair.

New members:—Mr. Stokes, Dr. Elder and Prof. Persifor Frazer, Jr., were introduced to the presiding officer and took their seats.

Letters accepting membership were received from Prof.